Looking Forward

The present world harvest of marine fishes, invertebrates, and algae is about 26.3 million metric tons a year. Disregarding economic and technical limiting factors, how much could that figure be increased? By a factor of two? Of four? Of ten? That is what people interested in developing marine resources would like most to know, for it bears heavily on the question of how much they should invest in efforts to achieve possible increases. Unfortunately there is simply not nearly enough of the right kind of quantitative information by which to calculate an answer which would have any dependable significance. The best we can do is to make a guess on the basis of (1) what people are now taking out of the sea in areas where fishery industries are relatively advanced, that is, North America, Europe, and Japan, (2) what we know about the geography of fertility and of suitable fishing weather, and (3) the location of harbors for fishing craft. Thus in making this estimate, we limit ourselves by present conceptions of the seas' productivity and by present knowledge of the seas' resources. We also limit ourselves to the present philosophies of exploiting the sea, to existing uses of marine organisms, to the kinds of animals and plants that are now used, to the known methods and equipment for producing them, and to the parts of the sea where it is economically and technically feasible to work with those methods and equipment. In the shadow of these limitations, it looks as though the total world production could increase in the natural course of events only by a factor of something less than two.

We can speculate more boldly about the seas' potential resources if we do not limit ourselves to tradition. As it is now, sea food the world over includes only a few species which are offered in a limited number of products. Suppose we think beyond these familiar things. Suppose we think of other possible products, such as protein flours, soil conditioners, pharmaceuticals, and insecticides. Suppose we imagine there existed some method of collecting sea animals and plants that did not depend on hunting or on catching with nets or hooks. As far as animals are concerned, it might mean herding them into a strategic spot with some such stimulus as sound, light, a chemical, or electricity. It might mean taking them out of the sea at that spot with pumps. Suppose all the brackish inshore waters were cultivated as farms for fish and invertebrates. Suppose that practical methods of improving salt-water environments were developed. This might mean fertilizing or feeding in the cultivated areas; it might mean destroying predatory animals of little value; it might mean installing artificial shelters for young fish. Suppose, too, that knowledge about genetics of marine organisms advanced to the point where it was possible to select for cultivation the stocks having the most desirable qualities in any given environment. Suppose the rates of harvesting all species were scientifically adjusted to provide the optimum yield of all, and suppose marine industries were flexible enough to permit shifting their attention effectively from one species to another. In other words, supposing that these industries were not hag-ridden by backward conventionalism as they are today, then the possibilities of enlarging the use of sea resources might expand very greatly.

Although in general the movement of marine industries is toward improvement, it is a drifting movement, not well aimed, and wasteful. To advance purposefully and aggressively requires fundamental knowledge about the biology and chemistry of organisms within the sea. Throughout the preceding chapters there are frequent references to the need for systematic biochemical assay of marine organisms. This is an essential precursor to developing a science dealing with marine resources after they have been gathered. It is essential to finding uses for species which are now neglected or wasted. It is essential to improving methods of preserving, storing, and transporting those which are now badly used.

One of the most important complexes of problems concerns the development of cheap methods of producing fishery products which are of uniform quality, keep well, and are easily shipped. This is needed in hot countries where marine faunas are composed of relatively small populations of many species and where catches are therefore heterogeneous. It is needed where refrigeration facilities are lacking, where transportation is slow and irregular, and where masses of people who are protein-starved live far inland. Perhaps the product that might best meet all these requirements is a protein flour made of marine organisms.[1]

The technological problems of making fish meal a cheap, acceptable, edible product of standard quality are not solved. There is much need for developmental research there. Perhaps this could best be conducted in a region where such a product is most needed, and by food technologists who are sympathetic with local tastes.

If edible fish meal were perfected and if people became willing to accept it, the demand for raw material would expand enormously. Then interest should turn to many species which are now neglected. Some of these are to be found in the family of the herrings and of the jacks, among certain deep sea fishes like the myctophids (which seem to be enormously abundant, but which we have yet to learn how to catch in large quantities), and among the thousands of species of invertebrate animals whose biochemical properties have never been assessed.

Large quantities of sea foods can be expected to occur in certain areas of the high seas where peculiarly favorable oceanographic mechanisms bring water to the surface from deep levels where great stores of nutrient salts have accumulated. The regions where such areas are known or expected to occur have been shown in Figure 14 (page 37). All of these need to be investigated. One of the regions that has been under extensive exploration by the United States Fish and Wildlife Service is the equatorial current system of the Pacific. These studies have demonstrated the existence of a band of water 400 to 600 miles wide (perhaps extending all the way across the Pacific Ocean) which is rich in nutrients, in plankton, and in oceanic pelagic fishes. Of particular interest in this zone are tunas, which are among the most valuable of all marine species. These are evidently abundant enough to support high seas fisheries, although much developmental work needs to be done to improve the efficiency and reduce the cost of harvesting. Other species that abound in this zone are wahoo, pomfret, dolphin, marlin, and squids. Similar zones occur elsewhere in the world and also remain to be explored and exploited.

The distribution of sperm whales may be a most useful guide to the location of these areas. Townsend,[2] studying log-book records from 1761 to 1920, has plotted the position of whaling ships on days when they took one or more animals. Occasional catches were widely scattered between latitudes 50°S. and 50°N. Within this zone, however, there are certain areas where whalers had the most consistent good luck (Figures 22 and 23, pages 261 and 262).

Sperm whales feed in deep water, mostly on squids and cuttlefish, including species which grow to gigantic size. Virtually nothing is known about the ecology of those great depths. The giant cephalopods must be exceedingly abundant to support the toothed whales. What supports *them?* There may be some association between good whaling grounds and good tuna grounds. At least in the Pacific the most extensive good sperm whale grounds are in a band across the ocean close to the equator but just a little south of the band of water which is rich in tuna. Very likely the two groups of animals are nourished in different parts of the same hydrographic system. If this is true, what about other places which sperm whales frequent, for example, the grounds northeast of New Zealand, the Japan ground, the extensive grounds of the North and South Atlantic, and the small grounds in the western part of the Indian Ocean? Most of these are in parts of the sea which have not been subjected to fishery exploration and they are not associated with existing great fisheries.

The question is, then, whether these grounds are associated with important untapped resources. We do not know. Until we do, our estimates of the potential production of the oceans of the world cannot be very significant. True, there are techniques of measuring the biological productivity in samples of water, but in the present stage of their development these techniques depend on the significance of the samples. They depend on our present conceptions of bionomics in the sea, which may be far from complete. They tell us little about what supports life in the deep levels beyond the range of ordinary photosynthesis, or about the abundance of harvestable useful organisms which, by grazing over wide areas and by aggregating into hordes or schools, concentrate organic material into relatively small space. Thus total biological productivity alone is not yet a satisfying measure of what and how much man can take out of the sea.

What, then, is a measure of it? How much can the yield of the sea be made to increase? The answer is, no one knows.

Does it seem possible that it could be increased enough to add significantly to the food supply in protein deficient areas? Certainly, but the size of the increase will depend on the solution of many problems. Some of these have been discussed in this book.

A great deal of money is now being spent on marine research. Could it be reallocated so as to fill in the existing gaps? No; there is no line of marine research which is being excessively supported. Indeed, with a few rare exceptions, the contrary is true. Impoverishing one kind of research to start another will contribute nothing to marine science.

It is true that scientists are never quite satisfied with what they have. They can generally use more—more assistants, more money, more field and laboratory equipment, more conferences, and so forth. However, in this particular field of science, considering the variations in all elements of the marine environments, the difficulties of observing and measuring them, the size of the oceans, the cost of operating vessels, the massive task of sorting, identifying, and counting biological collections and of analyzing chemical and physical material, of tabulating and working up quantitative data, and of studying all this to see what it means, too little support is given to marine research. True, fairly large amounts are spent in certain areas, but these are mostly for problems of sustaining, improving, or extending the existing great fisheries. Only negligible amounts are used for learning to understand the marine wilderness, or for pursuing new conceptions of full exploitation of its resources.

Suppose it were feasible to finance only one kind of activity. What would make the most significant contribution to expanding the use of ocean resources in a protein deficient region? In other words, it is very fine to be comprehensive, but perhaps that luxury cannot be afforded now. There is not enough time for that. Let us strike immediately at the heart of the problem.

Very well; where is that heart? Whatever kind of research is to be carried out will require scientists. There is a shortage of scientists. Perhaps training men to be scientists is the heart of the problem. We might establish small teaching centers in regions of critical need where resident faculties could give courses consisting of lectures, readings, and assignments. However, these courses should cover matters of particular application to the regions where the centers are situated. Therefore the faculties must have experience about their respective regions. Moreover the faculties must be able to keep alive intellectually. All this necessitates their conducting research in their regions. Therefore, there must be provision for some research as well as for teaching.

If the research is to have any bearing on the purpose of the centers, if it is to help the teachers lead in the advancement of fisheries in their regions, the research must deal with significant problems. Teaching alone, then, is not enough for the centers. There must be research too. If we were to concentrate on only one kind of research, what should that be? A thorough taxonomic and quantitative survey of the biological resources of each region would perhaps be the most useful thing to undertake. However it might be discovered early in such a survey that some of the animals in the area were poisonous (see Chapter 11). That is a very important problem in the tropics. That problem should be attacked before any use could be made of many of the species covered in the survey. Meanwhile the survey would find difficulty in catching some species of possible commercial value, which would necessitate inventing new kinds of fishing gear. Trial and error methods are a frequent approach to gear problems, but they are not efficient and not likely to advance fishing as a science. It would be much sounder to base these inventions on a knowledge of behavior of marine life. However, that would require special research, special scientists, and special equipment (Chapter 7).

It would be discovered in the course of a quantitative survey that the abundance of the marine organisms is not stable, that it oscillates and fluctuates. If investors are to be advised to start new industries, there had better be some sound information about the probable magnitude of these vacillations and about their causes. Consequently, these investors will need advice about the effects which fishing has on the abundance of the stocks, and if their operations are to be scientifically managed, they will need accurate year to year predictions of catches. Obviously such advice must be based on the results of systematic and environmental research (Chapters 5 and 6).

The survey would disclose a number of species which are abundant, sizable, and nutritious, and which therefore have possible commercial value. However, because these species are not used and are unfamiliar, methods of preparing and preserving must be devised and these would require technological studies. Here fundamental biochemical research would probably be more economical and fruitful in the long run than random kitchen-trials.

The centers might avoid these complications by specializing in a nonbiological subject such as vessel improvement. At first this seems like a good solution to the problem. However, the design of vessels for fishing must take into account the kind of fishing; therefore, the kind of fish; therefore, the habits of the fish, and so forth.

Thus, no matter where we attack the problem, we are forced eventually to conclude that the full use of sea resources depends on a web of knowledge. No thread of this web can be singled out as exclusively essential and no one knows enough about the sea to say which threads of the web can be safely ignored for the sake of economy. Nor does this knowledge stop with the sea. One of the most important fields for research is in principles of marine fishery management. Here I use the word "management" in a very broad sense, to include business management, direction of fishery industries, fishery conservation, and laws affecting fisheries. These topics are not independent of each other.

Comprehensiveness in marine biological research is *not* a luxury. It is a necessity which has been much neglected in the programs of most institutions. Our use of the sea as a source of food and other biological raw materials is technologically and philosophically about 200 years behind our use of the land. We have yet to search fully all the corners and depths of the seas, collect and identify all the species of its animals and plants, assay their biochemical composition, explore their possible uses, and develop efficient means of harvesting those worth using. We have yet to learn what controls the rates of their production, the laws governing the patterns of their behavior, or what we can do to manipulate environment to our advantage. And we have to learn how to put all this knowledge to profitable use as it develops. There is no reason to believe that the sea can meet all of the needs of humanity. Indeed, with present antiquated methods and philosophies of using its resources, there is no reason to believe that very much more can be added to present harvests.

But why put up with such antiquity? Why should not technologies and philosophies advance? If we learned how to use the sea resources fully and scientifically, the material rewards should well compensate for all the investment that would be required for the learning. There is no way of evaluating these rewards now. Nor can anyone honestly promise that they will in fact be forthcoming, any more than anyone could once have promised the ultimate use of atomic energy. There is no simple, direct short-cut to a full understanding of the proper uses of the sea. The most economical, efficient way to reach that understanding is not to try to do it penuriously. If we really desire to exploit the sea fully, if it is knowledge that we need to accomplish that purpose—and that is the theme of this book—then we had better make the necessary costly investment and put full effort into the job of acquiring that knowledge.